Glories.

MR. JAMES MCCONNEL asks in NATURE (vol. xl. p. 594) for acccounts of the colours and angular dimensions of glories. I saw a good instance of the phenomenon on Lake Superior, June 17, 1888, and, having had my attention called to the value of accurate descriptions in such cases by Mr. Henry Sharpe's "Brocken Spectres," I examined it carefully.

The shadow of my head on the mist was surrounded by a brilliant halo or glory, slaty-white around the head, followed by orange and red; then a circle of blue, green, and red, and the same colours repeated more faintly. The diameter of the innermost and brightest circle of red, as measured on the graduated semicircle of a clinometer, was $4\frac{1}{2}^{\circ}$. There was also a very distinct, but nearly white, fog-bow outside, of 42° radius, as measured in the same way. A. P. COLEMAN.

Faraday Hall, Victoria University, Cobourg, Ontario.

Fossil Rhizocarps.

REFERRING to Sir William Dawson's note on this subject in NATURE of November 7 (p. 10), we regret that we have been unable to trace the original source from which the statement in our "Hand-book of Cryptogamic Botany" was derived, relative to the fructification of *Protosalvinia* or *Sporangites*. The sentence will therefore, with apologies to Sir W. Dawson, be removed from future editions of the work.

ALFRED W. BENNETT.

The Arc-Light.

WOULD you or any of your readers kindly tell me where I may find an account of any of the latest methods of determining the back E.M.F. of the arc-light? JOSEPH MCGRATH.

Mount Sidney, Wellington Place, Dublin.

THE HYDERABAD CHLOROFORM COMMISSION.

THE appointment of a Commission at the present time to investigate the action of chloroform as an anæsthetic might to many seem an anomaly. For the use of chloroform as an anæsthetic was introduced over forty years ago: it was in November, 1847, that Prof. Simpson, of Edinburgh, first brought this valuable agent before the medical profession. Since that time, the use of chloroform has enormously extended, especially in our country, and although there are other valuable agents of the same class—such as ether and nitrous-oxide gas—yet there is a universality of opinion that the employment of chloroform has in many cases a special advantage. Considering the extensive use of the agent, and the progress which has been made of late years in the study of the action of drugs in man, it certainly is surprising that the knowledge of the effect of chloroform on the different parts and organs of the body is not complete. This is not altogether from want of attention to the subject; because, previous to the Hyderabad Commission, at least two Commissions were appointed with the view of investigating the action of chloroform and its occasional serious effects. These Commissions were appointed by the Royal Medical and Chirurgical Society of London, and by the British Medical Association, and they were composed of men who, from their knowledge of experiment and acquaintance with practical medicine, were competent to discuss the question. The two Commissions arrived at the same conclusions as the distinguished French man of science, Claude Bernard, had published years before, and these conclusions tallied with the teaching of the great London medical schools.

Chloroform and other anæsthetic agents have a peculiar position: they are powerful drugs used, not for disease itself, but for the purpose of allowing an operation to be performed, preventing the pain which would otherwise be felt, and relaxing the contraction and spasms of the muscles, so that the surgeon can more readily and accurately operate. The administration of the anæsthetic is something, then, outside the diseased condition; so that its use ought theoretically to be perfectly harmless to the sick person. Unfortunately it is not always so, and deaths from chloroform are, although rare, by no means unknown. The administrator of chloroform is therefore a person of great responsibility: he has to watch carefully the effect of the agent on the patient, to notice any unfavourable change that occurs, and to adopt measures to counteract any bad effects which appear. The knowledge of the mode in which chloroform causes danger to the life of the patient is therefore of vast importance; for, if the administrator knows the signs of danger, there is more likelihood of counteracting a fatal result. These fatal results, which are among the saddest that occur in medical practice, ought, if possible, to be avoided.

What, then, is the danger to life of chloroform? Or, to speak more fully, what particular part of the body does chloroform injuriously affect when there is danger? This is just the point that the various Commissions have attempted to settle. In the Scotch schools, more especially that of Edinburgh, it has been taught that the great danger of chloroform was in failure of respiration; meaning by this that the danger-signal of chloroform was the stoppage or irregularity of the breathing. As a corollary to this belief, it was considered that the heart was only affected after the breathing had become interfered with; that, in fact, the respiration stopping, the blood was not oxygenated, so the heart stopped beating. This was the teaching of the great Edinburgh surgeon, Syme. The English (and especially the London) teaching was almost directly opposed to this. It was taught, and is still taught in the London schools, that the great danger from chloroform arose from its effect on the heart, which stopped beating before the respiration ceased. Which, then, of these two doctrines is true, or are both true?

The decision of this question is, as we have stated, one of vast importance; but it must be remembered that, whichever is right, the administrator of anæsthetics always pays attention to both the beating of the heart and the regularity of the respiration. Surgeon-Major Lawrie, one of the prominent members of the Hyderabad Chloroform Commission, says that "it is possible to avert all risk to the heart by devoting the entire attention to the respiration during chloroform administration." Medical opinion in England, both of that of experts (professional anæsthetists) and of the general profession, is distinctly opposed to this view; and the administrator who does not attend to the pulse, as well as to the breathing, is certainly neglecting one of the main paths by which Nature shows us what is going on inside the organism.

From the statement of Surgeon-Major Lawrie just quoted, it will be seen that the Hyderabad Chloroform Commission came to the conclusion that the danger from the administration arose, not from the heart, but from the respiration. This view was strongly combated in our contemporary, the *Lancet*. The importance of the question led the Nizam of Hyderabad to obtain the services of a scientific medical man from England to go out to India and attempt to settle the question. Dr. Lauder Brunton, F.R.S., consented to go; and, well known as he is for his life-long devotion to the experimental investigation of the action of remedies and their practical application, it was considered probable that his aid in the research would lead to interesting and important results. From the somewhat scanty news of the results which have been telegraphed to England, it seems likely that the investigation now progressing at Hyderabad will tend to revolutionize existing views as to the action of chloroform.

Dr. Brunton's views as regards the dangers of chloroform before he left England were clearly expressed in his well-known "Text-book of Pharmacology." In it he says that "the dangers resulting from the employment of

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chloroform are death by stoppage of respiration and death by stoppage of the heart;" he lays as much stress on the effect on the heart as on the respiration, and he proceeds to affirm that too strong chloroform vapour may very quickly paralyze the heart. This view is, indeed, similar to the one we have already mentioned as taught in the London schools of medicine. It is also well known that death may occur soon after chloroform has begun to be administered, from the heart being affected. If the operation is begun too soon, fainting from pain may supervene, and a fatal result occur: this has always been strongly insisted upon by Dr. Brunton. Surgeon-Major Lawrie says that in such cases it is not the chloroform that acts on the heart, but simply that there is fatal syncope or fainting.

From the large number of experiments on animals which Dr. Brunton has performed in India, in conjunction with the Hyderabad Commission and a medical delegate of the Indian Government, it appears that the "danger from chloroform is asphyxia or an overdose;" there is none whatever from the heart direct. This statement is a distinct reversal of the view generally held in England. It means that chloroform causes a fatal result by affecting the respiration or by too much being taken into the system and affecting the brain; and that there is no direct paralysis of the heart from the chloroform. A perfectly impartial opinion cannot, however, be formed from the scanty records of the investigation which have been as yet received in England. We must wait for fuller details of the experiments before a final judgment can be passed.

It is well, however, to point out that the prevailing view in England has been founded, not only on experiments on the lower animals, but also on the extended clinical observation of two generations of medical men. Clinical observation is not so accurate or so lucid as that of direct experiment, but it has its value, and one by no means to be despised in a case where it is so extensive, and directed to a subject of such great importance, not only to the medical profession, but to the general public, as the question of the administration of chloroform.

ON THE CAVENDISH EXPERIMENT.

IN the last number of the Proceedings of the Royal Society (vol. xlvi. p. 253), I have given an account of the improvements that I have made in the apparatus of Cavendish for measuring the constant of gravitation. As the principles and some of the details there set out apply very generally to other experiments where extremely minute forces have to be measured, it is possible that an abstract of this paper may be of sufficient interest to find a place in the columns of NATURE.

In the original experiment of Cavendish (Phil. Trans., 1798, p. 469), as is well known, a pair of small masses, *m m* (Fig. 1), carried at the two ends of a very long but light torsion rod, are attracted towards a pair of large masses, M M, thus deflecting the arm until the torsion of the suspending wire gives rise to a moment equal to that due to the attraction. The large masses are then placed on the other side of the small ones, as shown by the dotted circles, and the new position of rest of the torsion arm is determined. Half the angle between the two positions of rest is the deflection produced by the attracting masses. The actual force which must be applied to the balls to produce this deflection, can be directly determined in dynamical units when the period of oscillation and the dimensions and masses of the moving parts are known. In the original experiment of Cavendish, the arm is 6 feet long, the little masses are balls of lead 2 inches in diameter, and large ones are lead balls 1 foot in diameter. Since the attraction of the whole earth on the smaller balls only produces their weight, *i.e.* the force with which they are attracted downwards, it is evident that the balls, M M, which are insignificant in comparison with the size of the earth, can only exert an extremely feeble attraction. So small is this that it can only be detected when the beam is entirely inclosed in a case to protect it from draughts; when, further, the whole apparatus is placed in a room into which no one must enter, because the heat of the body would warm the case unevenly, and so set up air currents which would have far more influence than the whole attraction to be measured; and when, finally, the period of oscillation is made very great, as, for instance, five to fifteen minutes. In order to realize how small must be the force that will only just produce an observable displacement of the balls, *m m*, it is sufficient to remember that the force which brings them back to their position of rest is the same as the corresponding force in the case of a pendulum which swings at the same rate. Now a pendulum that would swing backwards and forwards in five minutes would have to be about 20,000 metres long, so that in this case a deflection of one millimetre would be produced by a force equal to 1/20,000,000 of the weight of the bob. In the case of a pendulum swinging backwards and forwards once in fifteen minutes the corresponding force would be nine times as small, or 1/180,000,000 of the weight.

In spite of the very small value of the constant of gravitation, Cavendish was able, by making the apparatus on this enormous scale, to obtain a couple which would produce a definite deflection against the torsion of his suspending wire.

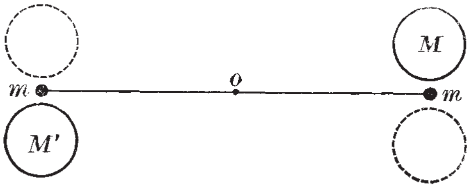

FIG. 1.

These measures were repeated by Reich (*Comptes rendus*, 1837, p. 697), and then by Baily (*Phil. Mag.*, 1842, vol. xxi. p. 111), who did not in any important particular improve upon the apparatus of Cavendish, except in the use of a mirror for observing the movements of the beam.

Cornu and Baille (*Comptes rendus*, vol. lxxvi. p. 954, vol. lxxxvi. pp. 571, 699, 1001) have modified the apparatus with satisfactory results. In the first place they have reduced the dimensions of all the parts to about one-quarter of the original amount. Their beam, an aluminium tube, is only $\frac{1}{2}$ metre long, and it carries at its ends masses of $\frac{1}{4}$ pound each, instead of about 2 pounds, as used by Cavendish. This reduction of the dimensions to about one-quarter of those used previously is considered by them to be one of the advantages of their apparatus, because, as they say, in apparatus geometrically similar, if the period of oscillation is unchanged, the sensibility is independent of the mass of the suspended balls, and *is inversely as the linear dimensions.* I do not quite follow this, because, as I shall show, if all the dimensions are increased or diminished together, the sensibility will be unchanged. If only the length of the beam is altered and the positions of the large attracting masses, so that they remain opposite to, and the same distance from, the ends of the beam, then the sensibility is inversely as the length. This mistake—for mistake it surely is—is repeated in Jamin's "Cours de Physique," tome iv. ed. iv. p. 18, where, moreover, it is emphasized by being printed in italics.

The other improvements introduced by Cornu and

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